

It is certainly very far from my desire to discourage the present attempts which are being made to clear the atmosphere of our large towns of smoke, and I have recognised the advantages which would result from the adoption of more perfect forms of combustion. In my paper I have simply distinguished between fogs and smoke, and separated them for distinct consideration and treatment, and have at the same time directed attention to some points which ought to be considered before deciding on their prevention.

With regard to Mr. Russell's difficulty in reconciling the result of the experiments with what is observed with regard to fogs in London, Paris, and other large towns, it appears to me to have arisen entirely from not putting sufficient weight on the all-important influence of the amount of vapour in the air of the different places. It is condensed vapour which forms the fog, and dust simply determines whether it will condense in fine- or coarse-grained particles. The atmosphere of Paris, compared with that of London, is an extremely dry one, and the air is seldom in a condition to produce fogs. The atmospheres of the other towns mentioned are also drier, some of them very much drier, than that of London. London however will probably be always more subject to fogs than other cities on account of its great size, some part of it being always in its own smoke.

Considered from a different point of view, might not the fog of January 31, 1880, referred to by your correspondent, be cited in evidence of a conclusion the opposite of that drawn by the writer, and in favour of the correctness of the experimental results? From this point of view the low white fog cleared away because it was formed in the comparatively pure air of the streets, while the higher fog did not clear away because it was formed in the products of combustion. The true explanation however would rather appear to be, that where the fog was white it was also of less depth than in those places where it "extended high" and mixed with the smoke; and the sun, which was only sufficient to dispel the lesser depth "more or less," would evidently be insufficient to clear away the greater depth. It is however impossible to form any definite idea as to how this particular fog conducted itself, without much fuller information as to air-current, &c.

I have communicated to the secretary of the Royal Society of Edinburgh a second experimental paper on fogs, with special reference to dry fogs. In this paper the full answer to the latter part of Mr. Russell's letter will be found. JOHN AITKEN

Darroch, Falkirk, January 24

Professors Exner and Young

MY statement in respect to Prof. Exner's having announced the thermo-electric neutrality of a bismuth-antimony pair immersed in pure nitrogen, rested upon a note in NATURE (vol. xxii. p. 156), and this it seems was based upon a statement in *L'Electricité*. I have seen those of Prof. Exner's papers which have appeared in the *Annalen der Physik*, and there is certainly nothing of the sort in them; but I supposed that it must be contained in some other paper in some one of the numerous other publications to which I have not access here. It never occurred to me, until within a very short time, that there could be any mistake as to his having made such an assertion. How or where the error originated I cannot quite understand; but I trust Prof. Exner will accept my apologies for my share in its propagation, and that he and all concerned will be satisfied that no misrepresentation was intended on my part. The incident is a good illustration of the extreme care necessary in commenting upon the views of another person. C. A. YOUNG

Princeton, U.S.A., January 12

The Flying-fish

IT is remarkable that there should still be any doubt as to the facts in connection with the flight of the flying-fish. Dr. Günther ("Study of Fishes," p. 622), summarising the observation of Möbius, says that "they frequently overtop each wave, being carried over it by the pressure of the disturbed air" (in the open sea!). Again, flying-fishes "never" fall on board vessels "during a calm or from the lee side." At night "when they are unable to see they frequently fly against the weather-board, when they are caught by the current of air and carried upwards to a height of twenty feet above the surface of the water." Surely the fish going at the rate of at least ten miles an hour would on striking the "weather-board" be dashed, bruised

and helpless, back into the water instead of coming over the side fresh and vigorous, flapping about on the deck. Except when "by a stroke of its tail" it turns towards the right or left, Möbius concludes that "any deflection from a straight course is due to external circumstances, and not to voluntary action on the part of the fish."

I have watched flying-fish repeatedly, and have invariably seen them fly, or rather glide, over the surface of the sea, and from one to two feet above it, rising gently to the swell when there was no wind, and occasionally turning to the right or left without touching the water. I do not say that when there is a breeze the tail of the fish may not touch it, but I think that, with the foam and spray of the broken water, it would be very difficult to be sure of it, and, moreover, if the tail was used the motion would be a jerking one. Mr. Wallace speaks of their "rising and falling in the most graceful manner," which, although he is referring to another species, applies also to the North Atlantic form (*Exocoetus evolvans*). Mr. Bennett ("Gatherings," &c., p. 14) says that they "spring from the sea to a great elevation." This is probably in reference to their coming on board ship at night, attracted, it is supposed, by the lights. I believe the pectoral fins are kept extended without any motion, except perhaps as Mr. Whitman,¹ a recent observer, says, just when they rise from the sea. He gives 800 to 1200 feet as the greatest distance he has seen them fly, and about forty seconds as the longest time out of the water. By what mechanical means they move when out of the water is still to me a mystery.

I have never known the flying-fish to be pursued by other fish, nor ever seen any bird near them; indeed few birds are ever seen far from the land north of the southern tropic, where flying-fish are most abundant. The dolphin (*Coryphæna*) is supposed to be their greatest enemy. I had once an opportunity of seeing one opened—in the West Indies—its stomach was quite full of *Orthogoriscus nola*, very young, being not quite an inch long.

FRANCIS P. PASCOE

1, Burlington Road, W., January 21

Mr. S. Butler's "Unconscious Memory"

I MUST reply to the review of my book, "Unconscious Memory," in your issue of the 27th inst., and to Dr. Krause's letter on the same subject in the same issue.

Mr. Romanes accuses me of having made "a vile and abusive attack upon the personal character of a man in the position of Mr. Darwin," which I suppose is Mr. Romanes' way of saying that I have made a vile and abusive personal attack on Mr. Darwin himself. It is true I have attacked Mr. Darwin, but Mr. Romanes has done nothing to show that I was not warranted in doing so. I said that Mr. Darwin's most important predecessors as writers upon evolution were Buffon, Dr. Erasmus Darwin, Lamarck, and the author of the "Vestiges of Creation." In the first edition of the "Origin of Species" Mr. Darwin did not allude to Buffon nor to Dr. Erasmus Darwin, he hardly mentioned Lamarck, and he ignored the author of the "Vestiges" except in one sentence. This sentence was so gross a misrepresentation that it was expunged—silently—in later editions. Mr. Romanes does not and cannot deny any part of this.

I said Mr. Darwin tacitly claimed to be the originator of the theory of evolution, which he so mixed up with the theory of "Natural Selection" as to mislead his readers. Mr. Romanes will not gainsay this. Here is the opening sentence of the "Origin of Species":—

"When on board H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the inhabitants of South America, and in the geological relations of the present to the past inhabitants of that continent. These facts, as will be seen in the latter chapters of this volume, seemed to throw some light on the origin of species; that mystery of mysteries, as it has been termed by one of our greatest philosophers. On my return home it occurred to me in 1837 that something might perhaps be made out on this question by patiently accumulating and reflecting upon all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate upon the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions which then seemed to me probable; from that period to the present day I have steadily pursued the same object. I hope that I may be

¹ See *Zoologist* for November, 1880.

excused for entering upon these personal details, as I give them to show that I have not been hasty in coming to a conclusion." — "Origin of Species," p. 1, ed. 1859.

What could more completely throw us off the scent of the earlier evolutionists, or more distinctly imply that the whole theory of evolution that follows was an original growth in Mr. Darwin's own mind?

Mr. Romanes implies that I imagine Mr. Darwin to have "entered into a foul conspiracy with Dr. Krause, the editor of *Kosmos*," as against my book "Evolution, Old and New," and later on he supposes me to believe that I have discovered what he calls, in a style of English peculiar to our leading scientists, an "erroneous conspiracy." The idea of any conspiracy at all never entered my mind, and there is not a word in "Unconscious Memory" which will warrant Mr. Romanes' imputation. A man may make a cat's paw of another without entering into a conspiracy with him.

Later on Mr. Romanes says that I published "Evolution, Old and New," "in the hope of gaining some notoriety by deserving, and perhaps receiving, a contemptuous refutation" from Mr. Darwin. I will not characterise this accusation in the terms which it merits.

I turn now to Dr. Krause's letter, and take its paragraphs in order.

1. Dr. Krause implies that the knowledge of what I was doing could have had nothing to do with Mr. Darwin's desire to bring out a translation of his (Dr. Krause's) essay, inasmuch as Mr. Darwin informed him of his desire to have the essay translated "more than two months prior to the publication of" my book, "Evolution, Old and New." This, I have no doubt, is true, but it does not make against the assumption which I made in "Unconscious Memory," for "Evolution, Old and New," was announced fully ten weeks before it was published. It was first announced on February 22, 1879, as about to contain "copious extracts" from the works of Dr. Erasmus Darwin and a comparison of his theory with that of his grandson, Mr. Charles Darwin. This announcement would show Mr. Darwin very plainly what my book was likely to contain; but Dr. Krause does not say that Mr. Darwin wrote to him before February 22, 1879—presumably because he cannot do so. I assumed that Mr. Darwin wrote somewhere about March 1, which would still be "more than two months before" the publication of "Evolution, Old and New."

2. Dr. Krause says I assume that "Mr. Darwin had urged him to insert an underhand attack upon him (Mr. Butler)." I did not assume this; I did not believe it; I have not said anything that can be construed to this effect. I said that Dr. Krause's concluding sentence was an attack upon me; Dr. Krause admits this. I said that under the circumstances of Mr. Darwin's preface (which distinctly precluded the reader from believing that it could be meant for me) the attack was not an open, but a covert one; that it was spurious—not what through Mr. Darwin's preface it professed to be; that it was antedated; that it was therefore a spurious and covert attack upon an opponent interpolated into a revised edition, the revision of which had been concealed. This was what I said, but it is what neither Mr. Romanes nor Dr. Krause venture to deny. I neither thought nor implied that Mr. Darwin asked Dr. Krause to write the attack. This would not be at all in Mr. Darwin's manner.

3. Dr. Krause does not deny that he had my book before him when he was amending his article. He admits having taken a passage from it without acknowledgment. He calls a page and a half "a remark," I call it "a passage." He says he did not take a second passage. I did not say he had; I only said the second passage was "presumably" taken from my book, whereas the first "certainly" was so. The presumption was strong, for the passage in question was not in Dr. Krause's original article; it was in my book, which Dr. Krause admits to have had before him when amending his article, and it came out in the amended article; but if Dr. Krause says it is merely a coincidence, of course there is an end of the matter.

4. Dr. Krause, taking up the cudgels for Mr. Darwin, does not indeed deny the allegations I have made as to the covertness, and spuriousness, and antedating of the attack upon myself, but contends that "this is not due to design, but is simply the result of an oversight"; he is good enough to add that this oversight "could only be most agreeable" to myself. When I am not in the wrong I prefer my friends to keep as closely as they can to the facts, and to leave it to me to judge whether a modification of them would be "most agreeable" to me or no. What, I wonder, does Dr. Krause mean by oversight? Does he mean

that Mr. Darwin did not know the conclusion of Dr. Krause's essay to be an attack upon myself? Dr. Krause says, "To every reader posted up in the subject this could not be doubtful," meaning, I suppose, that no one could doubt that I was the person aimed at. Does he mean to say Mr. Darwin did not know he was giving a revised article as an unrevised one? Does he mean that Mr. Darwin did not know he was saying what was not true when he said that my book appeared subsequently to what he was then giving to the public? Does he pretend that Mr. Darwin's case was not made apparently better and mine worse by the supposed oversight? If the contention of oversight is possible, surely Mr. Darwin would make it himself, and surely also he would have made it earlier? Granting for a moment that an author of Mr. Darwin's experience could be guilty of such an oversight, why did he not when it was first pointed out, more than twelve months since, take one of the many and easy means at his disposal of repairing in public the injury he had publicly inflicted? If he had done this he would have heard no more about the matter from me. As it was, he evaded my *gravamen*, and the only step he even proposed to take was made contingent upon a reprint of his book being called for. As a matter of fact a reprint has not been called for. Mr. Darwin's only excuse for what he had done, in his letter to myself, was that it was "so common a practice" for an author to take an opportunity of revising his work that "it never occurred" to him to state that Dr. Krause's article had been modified. It is doubtless a common practice for authors to revise their work, but it is not common when an attack upon an opponent is known to have been interpolated into a revised edition the revision of which is concealed, to state with every circumstance of distinctness that the attack was published prior to the work which it attacked.

To conclude: I suppose Mr. Romanes will maintain me to be so unimportant a person that Mr. Darwin has no call to bear in mind the first principles of fair play where I am concerned, just as we need keep no faith with the lower animals. If Mr. Darwin chooses to take this ground, and does not mind going on selling a book which contains a grave inaccuracy, advantageous to himself and prejudicial to another writer, without taking any steps to correct it, he is welcome to do so as far as I am concerned—he hurts himself more than he hurts me. But there is another aspect of the matter to which I am less indifferent: I refer to its bearing upon the standard of good faith and gentlemanly conduct which should prevail among Englishmen—and perhaps among Germans too. I maintain that Mr. Darwin's recent action and that of those who, like Mr. Romanes, defend it, has a lowering effect upon this standard. S. BUTLER

Geological Climates

WHEN a reader of the intelligence of Mr. Wallace misunderstands my words it becomes plain to me they have failed to convey my meaning. I do not accept the interpretation he has put upon them, nor do I admit that even that interpretation would tell so much in favour of his theory as he supposes.

As however I agree with him that the question is far too large to be fully discussed in your columns, I shall allow the controversy, so far as I am concerned, to terminate, and shall publish my detailed views on geological climate in another way. SAMUEL HAUGHTON

Trinity College, Dublin, January 27

On the Spectrum of Carbon

IN the discussions on the spectrum of carbon which have recently appeared in your journal much stress is laid on the impossibility of volatilising that substance by any heat which man can produce. I think this assumption is not warranted by experience. Two or three facts in Despretz' account of a remarkable set of experiments which he made about thirty years ago, seem to me to show it to be unfounded. This is given in the *Comptes rendus*, vol. xxviii. He exposed rods of anthracite to the action of 125 Bunsens (zincs $5\frac{1}{2}$ in. high) and also to the solar focus of an annular lens 36 in. diameter. The rods bent under the combined action, and even appeared to fuse! In vol. xxix. he describes experiments with rods of sugar-charcoal under a battery of 500 similar cells. The electric egg was covered suddenly with a hard block crystalline powder.

He thinks attempts to fuse carbon should be made in condensed nitrogen and in metallic vessels. In the same volume he says that with 600 cells rods of sugar charcoal bend—swell at the